

TELEPHONE 87583.

STRANGWAYS RESEARCH LABORATORY,
WORT'S CAUSEWAY,
CAMBRIDGE.

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Dr Joshua Lederberg
Dept. of Genetics
University of Wisconsin

Dear Dr Lederberg,

Thank you very much for your interesting reprints from which I learned a great deal. To show you how I appreciate your review I should like to draw your attention to a few minor defaults, hoping you will accept them as dictated by good will.

The field you review in "Cell Genetics and Hereditary Symbiosis" is so vast that you could not possibly peruse every publication, particularly older ones, and thus I am not surprised that you made use of Lwoff's writings. They may be called stimulating and even brilliant but certainly not objective, and you should not look too much through his spectacles but read him critically. The paragraphs on *Euglena* on pp. 406 and 407 give not a true picture of the situation and do not sufficiently integrate the various results, nor do they differentiate between findings and hypotheses. Perhaps you would do me the favour and read my papers again, also that of 1937 (*Beiträge zur Physiologie saprotropher Algen und Flagellaten III, Planta* 27 61,).

To my knowledge no other alga has been "bleached" so far by streptomycin than *Euglena gracilis*. *Chlorogonium* looks almost colourless in every young thriving culture in a rich medium; - nor are chromatophores of *Euglena gracilis* destroyed by rich feeding. Not the "fate of the pyrenoid" is

controversial but the existence of chromatophores where none can be demonstrated. Temperature treatment causes destruction of chromatophores as does streptomycin. That is essential! Perhaps you find the time to inspect pp. 213 - 216 in Lwoff's book of 1943. It's all wrong!

In your treatment of *Paramaecium bursaria* as at other places you give several authors in one pair of brackets, so that the reader does not know to whom the various findings are attributable.

I am sure you can easily mend these small mistakes in your next review, and then they will be still more useful. We need people who take such troubles.

Some more reprints will be send under separate cover.

Yours sincerely

E.G. Pringsheim

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